

necessary to use the most rigorous means to calculate the position by dead reckoning, so that the errors of steering, &c., may not be augmented by errors in calculation. Such being the case, we regret that Mr. Merrifield has omitted from the chapter on traverse sailing the warning given in Raper that, especially in high latitudes, the differences of longitude should be found on each course, instead of the departures being lumped and the difference of longitude found from the result.

In the chapter on soundings and tides (No. 10), Mr. Merrifield has published the system of the late Sir Francis Beaufort for ascertaining the height of the tide at any moment provided we know the range and time of high water. This is the method generally adopted by surveyors when circumstances prevent their having a tide pole on shore, and is traditionally known amongst them, though not hitherto published. It is fairly accurate when the diurnal inequality is inconsiderable, and we can recommend it as being sufficient for all practical purposes in finding the depth of water to be added to the soundings on the chart in places like the Bristol and Irish Channels, where it is necessary, owing to the large ranges, to take the state of the tide into consideration in judging the position by soundings in foggy weather, or in calculating when a bank or flat can be safely crossed. The fact that in rivers or harbours certain winds affect the height and that atmospheric pressure also has an influence over tides may be safely ignored in the open sea, as their combined influence would probably never exceed half a fathom, but a range of from three to five fathoms can never be lightly considered by the careful navigator.

OUR BOOK SHELF

Farm Insects. Being the Natural History and Economy of Insects Injurious to Field Crops, and also those which Infest Barns and Granaries, with Suggestions for their Destruction. By John Curtis, F.L.S. Pp. 540, with 16 Coloured Plates, Royal 8vo. (London : John Van Voorst, 1883.)

THIS is simply a reissue of Curtis's classical work ; it had long been "out of print" in booksellers' phraseology. It remains the best book on economic entomology that has appeared in this country, and has certainly served as a model for the Reports of various State entomologists on the other side of the Atlantic. No other author here has gone into the question of special injurious insects with the same care and minuteness, and it may be said that (with the exception of certain Reports issued in America) there is no similar collective work faithfully illustrated by the author's own pencil. The plates and woodcuts are in Curtis's best style, and if he had been an entomological artist only, his work would have remained unsurpassed.

Opinions may be divided as to the desirability of reissuing such a work "untouched," when so many years have elapsed since the publication of the chapters in the *Proceedings of the Royal Agricultural Society* that formed its basis. Much and valuable additional information has been obtained since the original articles were written, and very much alteration in nomenclature has resulted from the efforts of systematists to place this branch of entomological science on a sounder footing, but the facts remain practically unaltered, and there is the charm of a certain originality in the author's style that any radical reconstruction might have destroyed.

Nevertheless we do think it a pity that some one could not have been found with sufficient knowledge and courage to re-edit the book and bring it down to date. On the

other hand, this process might have resulted in the work being no longer "Curtis's Farm Insects." Its value would be destroyed if rewritten, even by the most experienced, and we think the only practicable method of dealing with it in an absolutely new edition would be by means of copious annotations, not by recasting the whole.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Evolution of the Cetacea

I AM glad to be able to assure Mr. Searles Wood that I have long been familiar with the specimen called *Paleocetus sedgwicki*, preserved in the Woodwardian Museum at Cambridge, and have repeatedly examined it with much interest. It is undoubtedly Cetacean, and allied to the genus *Balaenoptera*, as Mr. Seeley demonstrated, though differing in smaller size and some other characters from any existing species. As, however, the light it throws upon the evolution of the Cetacea is very small compared to the time that would have been taken up in discussing its bearings, I did not think it worth while to allude to it in a lecture of which the length was necessarily limited. It is, after all, a most unsatisfactory fragment, as its geological age is, and probably always will remain, a matter of doubt. Allowing, however, the utmost antiquity assigned to it, my argument would rather be strengthened than weakened. Mr. Searles Wood seems to have missed the fact that my chief contention was against the prevalent view that the Cetacea have been derived from the Carnivora through the Seals. Any evidence which throws back their origin in time and derives them from some more generalised type of mammals would militate against this view. No one can suppose that the Ungulata originated at the commencement of the Tertiary period, as we know that they were then already differentiated into great and distinct sections. Their primitive ancestry must therefore be looked for far back in Mesozoic times. That I thought the Cetacea existed before the Tertiary period I distinctly intimated by suggesting, as an explanation of the absence of their remains in the chalk, that they might then have been inhabitants of great inland waters, but having had so many warnings of the fallacy of negative evidence in geology, I do not yet despair of the discovery of a veritable Cretaceous whale.

W. H. FLOWER

The Java Eruption

I HAVE been greatly interested in your note on M. Renard's researches as to the composition of the volcanic material ejected during the recent eruption of Krakatoa. The ashes, as stated, are those of a magma that would have produced an andesite with rhombic pyroxene. Now such an andesite occurs at so many points, and in such immense masses, round the great Pacific "circle of fire," that one is tempted to ask if it may not specially characterise this important volcanic region. I will, with your permission, briefly refer to some published, and one or two unpublished, facts with regard to the distribution of this andesite (called hypersthene-andesite by Whitman Cross and Iddings, and bronzite-andesite by F. Becke) round the Pacific circle.

In the *Neues Jahrbuch* for 1881 (Beilage Band 1881, 467) Dr. Oebbeke describes, under the term augite-andesite, a rock from the Sierra de Mariveles, Luzon. Owing to the kindness of the author, I have a section of this rock before me as I write, and I have little doubt that the strongly pleochroic mineral is mainly, if not entirely, a rhombic pyroxene. Augite, however, is also present.

Passing to the other side of the Atlantic, we have recent evidence to show that a rock of the same type occurs along the line of the Rocky Mountains and the Andes.

In *Bulletin No. 1 of the U.S. Geological Survey* (1883), Mr. Whitman Cross describes a hypersthene-andesite from Buffalo Peaks, Mosquito Range, Colorado.

In the *American Journal of Science* for September, 1883, Messrs. Hague and Iddings prove that the four great volcanic peaks of Mount Rainier, Mount Hood, Mount Shasta, and Lassen's Peak, rising to heights of from 10,500 to 14,444 feet above sea-level in California, Washington Territory, and Oregon, are mainly composed of andesitic lavas and tuffs, in which hypersthene is the predominating bisilicate.

In the *Geological Magazine* for July, 1883, Mr. Waller describes a similar rock from Montserrat, and I have just analysed one for Prof. Bonney from Old Providence Island in the Caribbean Sea. Prof. Bonney also informs me that he has found the rhombic pyroxene in the andesites brought by Mr. Whymper from Pichincha and Antisina.

It must not, however, be supposed that the rock is limited either to the Pacific region or to the Tertiary and Recent periods.

M. Fouque has shown that hypersthene occurs in the Santorin lava of 1866.

Niedzwiedski described a hypersthene-andesite from Steiermark in 1872. Mr. Whitman Cross and myself have recognised the rhombic pyroxene in many well known Hungarian rocks, in which it had previously been regarded as augite. Lastly, thanks to kind assistance rendered by Prof. Rosenbusch, I have been enabled to show that some Palæozoic lavas and tuffs of the Cheviot region are of essentially the same type (*Geol. Mag.*, March, June, and August, 1883).

J. J. HARRIS TEALL

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Diffusion of Scientific Memoirs

PROF. TAIT'S admirable remarks on the moral obligation laid upon "every society whose memoirs are worthy of appearing in print" to disseminate its publications must have awakened a cordial response in the minds of many whose lot is cast in some provincial city or outlying local college. It is only too true that the volumes of the *Cambridge Philosophical Transactions* are "almost inaccessible" to many like myself, who often find themselves tantalised by the desire of consulting some of the classical masterpieces of research or analysis therein enshrined, which, therefore, are not to be consulted without a pilgrimage to Cambridge or to London. Yet I hardly understand why Prof. Tait should—save for the occasion of reviewing the happily exhumed memoirs of Prof. Stokes—have chosen the *Cambridge Transactions* as the one instance of "inaccessibility," since it is at least equally to be regretted that a memoir published in the *Transactions of the Royal Society of Edinburgh*—and there are masterpieces of research and analysis by the score irrevocably buried therein—equally necessitates a pilgrimage on the part of the provincial reader. I, for one, shall be extremely glad if Prof. Tait will act upon his own prescription—that simple, easy cure—and consider himself "bound to disseminate as widely as possible" the memoirs which he has himself consigned to those very inaccessible *Transactions*. I doubt, indeed, if even Prof. Tait has realised the difficulty besetting a would-be reader of original memoirs and researches, who is compelled to journey from one shore of England to the other in order to consult the *Edinburgh Transactions*, the *Cambridge Transactions*, the *Comptes Rendus*, the volumes of *Poggendorff's Annalen*, or those of the *Annales de Chimie et de Physique*, or the memoirs of any one of the five great Academies of the European Continent.

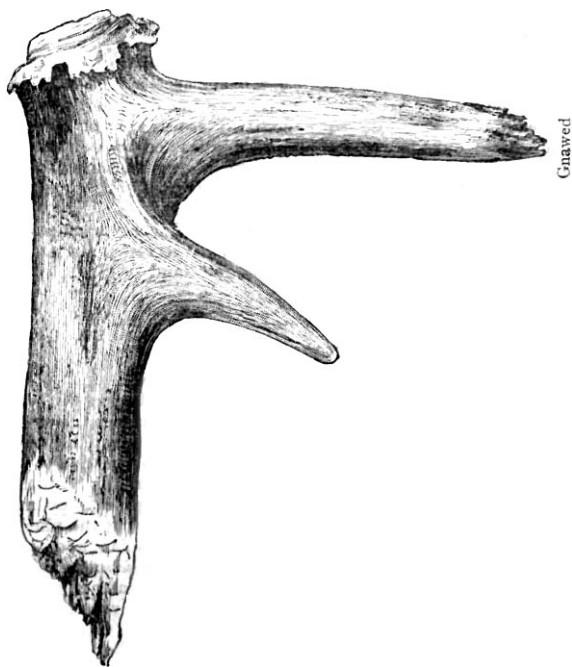
SILVANUS P. THOMPSON

University College, Bristol, December 14

Deer and their Horns

THE question is often asked, What becomes of the horns shed every year in the deer forests? the number picked up or found hardly accounts for all those which have been shed. It is said that the deer themselves eat them. It is difficult to conceive how a deer, with its toothless upper jaw, can eat a hard bone—for such is a shed horn—but it seems probable, nevertheless, that they do so. I picked up a horn recently in the deer forest at Dunrobin which appears to show that it has been in great part eaten away; and this, I think, was the opinion of the members of the Zoological Society to whom I exhibited it last Monday. On inquiry from the head-keeper at Dunrobin, Mr. James Inglis, I find that it is the general belief that the deer do eat the shed horns, whilst the appearance of the specimen here referred to, confirms the popular belief. The marks on it are such as would be made by the broad incisors of the lower jaw, and the appearance generally suggests that the horn has been

gnawed and munched by the cutting teeth of the lower and the toothless gums of the upper jaw. It would appear, therefore, I think, that deer do eat some at least of the shed horns, and this



Red deer's horn, eaten (by other deer?), picked up in deer forest, Sutherland, 1883. A young stag's horn.

is rendered the more probable by the fact, according to Mr. Inglis, that there are no foxes or other animals in this particular forest to account for the mischief.

J. FAYRER

December 8

"I BEG leave to inform you that I am unable to say from personal knowledge whether it is the stags or hinds that eat the shed horns in the forest. I have never seen either eating horns, but I have no doubt they do so, probably both stags and hinds.

"I have never known dogs to eat deer-horns, and we have no foxes in our forest, and very rarely any dogs are to be seen in it; 'even although they should eat them,' the number of pieces we find all the year round, nearly all partially eaten, leaves no room to doubt that no other animal could have eaten them. I think they commonly eat them after they have been lying exposed to the weather for some time; the horns are then softer from exposure.

"In every case that I have seen, they commence at the top or point of the horn, and eat down towards the root or burr; the latter part is often left uneaten. As soon as I can collect a few specimens I will send them to you.

"We often find horns entire without any marks of teeth on them, but those are mostly not long shed. I have also got horns that had apparently lain for years without any marks on them. But of course no one would expect all the shed horns to be eaten.

"I am sorry that I cannot give you more information, and I am also sorry that as yet I have not been able to collect more information than I know myself, but when I have any fresh evidence I will let you know.

"JAMES INGLIS

"November 18"

Sprengel on the Fertilisation of Flowers

IN NATURE, vol. xxix. p. 29, is a letter from Prof. Hagen of Cambridge, Mass., calling attention to the fact that Sprengel's treatise on the structure and fertilisation of flowers was not unappreciated in his own day. Now it so happened that only a week or two before reading this I took up by chance the "Introduction to Physiological and Systematical Botany," by Sir James Edward Smith, the American edition, dated 1814. On p. 208 the author says:—